## LETTERS TO THE EDITOR.

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### Relation between Uranium and Radium in some Minerals.

IN the course of an investigation which I am conducting on uranium-bearing minerals, the detailed results of which will be published shortly, I have come upon a point which seems to be of sufficient interest to warrant immediate publication. This is the close agreement between the amount of uranium and the amount of radium present in those minerals which have been examined.

The method which has been employed is briefly as follows :—A weighed quantity of the powdered mineral is introduced into a small glass bulb, which is connected with a larger bulb by a short tube. Attached to the bulb containing the mineral is another small bulb containing a small quantity of a suitable acid. The whole apparatus is sealed up at a slightly diminished pressure, and by tilting, the acid is brought into the bulb with the mineral, and the complete decomposition of the latter effected by gentle heating. At the end of a couple of days the larger bulb is sealed off from the smaller and allowed to stand for two hours to permit any rapidly decaying emanation which it may contain to dissipate. A small quantity of strong sodium hydroxide solution is introduced into the bulb, and the walls are thoroughly wetted in order to remove the acid fumes. The air contained in the bulb is then transferred to an air-tight electroscope and the rate of leak measured. In comparing the results obtained with different minerals the rate of leak at the end of three hours has been chosen, since at this time the rate of decay of the excited activity and its rate of formation are in equilibrium, and the readings of the electroscope are constant over considerable periods. The quantity of uranium in the solution is determined by analysis, and the ratio between the volumes of the two parts of the apparatus determined by measuring their separate capacities.

The results which have been obtained are as follows :—

| No. | Substance | Per cent. Uranium | Grams Uranium taken | Leak divisions per min. | Ratio leak to Uranium |
|---|---|---|---|---|---|
| 1 | Uraninite | 82·5 | 0·1067 | 22·5 | 211 |
| 2 | Gummite | 66·1 | 0·0982 | 20·8 | 212 |
| 3 | Uranophane | 46·6 | 0·0671 | 12·1 | 181 |
| 4 | Uraninite | 83·9 | 0·0994 | 20·6 | 207·2 |
| 5 | Samarskite | 9·5 | 0·0292 | 6·4 | 218 |

Nos. 1, 2, 3 and 5 from North Carolina; No. 4 from Branchville, Conn.

The slightly low value of the constant in No. 3 can be explained by the fact that this mineral at ordinary temperature gives off constantly a small proportion of its emanation, and is therefore not in complete equilibrium.

These results show a direct variation from those obtained by Mr. Strutt (*Proc.* Roy. Soc., lxxiii., 191), which may perhaps be explained by the fact that he secured the emanation by heating his minerals. Experiments which I have made show that on heating samarskite to low redness only 10 per cent. of the total emanation is given off, and that heating to bright redness releases only 20 per cent. of the total emanation obtained when the very finely powdered mineral is completely decomposed by heating with concentrated sulphuric acid. BERTRAM B. BOLTWOOD.

139 Orange Street, New Haven, Conn., U.S.A., May 7.

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### The Source of Radium.

As the subject of the origin of radium is being discussed, I may perhaps be permitted to make a suggestion.

The source of radium is at present being looked for on the assumption that it is the disintegration product of a substance of higher atomic weight. If this is so, we have apparently to choose between uranium and thorium. Mr. Soddy's experiment throws serious doubts upon the former being the source. The products of the latter appear to have been sufficiently far traced to render it doubtful that it can be the source. Again, the element thorium seems to be scarce in, or even absent from, some radio-active pitchblendes, if the older analyses are to be relied upon.

I would suggest that radium may not be derived purely as a disintegration product, but as an atomic combination of radio-active products with some of the elements present in pitchblende. Thus radium would represent the synthesis of an element, not its decomposition. On this view some of the radio-active products of uranium (or thorium) can, in virtue of their great kinetic energy, enter into the atoms of intermixed substances, such as barium, bismuth, &c., giving rise to the new atom radium. The new atom is, however, not very stable, and is consequently short lived. Hence its radio-activity.

If this hypothesis is correct, we should seek to observe the genesis of radium not in any one of the radio-active elements, but in molecular intermixtures of these with the various bodies we know to be conspicuously present in pitchblende, seeking among the various combinations for a positive result. The quantities of radium (or its emanation) to be expected as generated in a given time remain, of course, the same as on the hypothesis of disintegration; thus the experimental investigation presents no additional difficulties beyond its greater prolixity. J. JOLY.

Trinity College, Dublin, May 17.

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As Mr. Soddy is absent from England, it may be permitted to me to comment on Prof. Joly's letter. The idea had occurred to us; but, as remarked, the experiments would be very "prolix."

A more promising field of research appears to be to try to ascertain whether the immense amount of energy evolved in various forms during the disintegration of the radium emanation may not be able to cause chemical change of a constructive nature; for example, to change bromine into iodine. An attempt has been made to see if this was the case, but without a positive result. That iodine would be the product of an addition of energy to bromine is of course a mere guess; but iodine is easily tested for, and hence the experiment. The difficulty will be in recognising with certainty the product of any such change, for the quantity of matter to be produced is, of course, extremely small. It is only, however, by such "mad" experiments that the capabilities of the radio-active bodies can be ultimately gauged. WILLIAM RAMSAY.

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### Radio-activity of Russian Muds and Electrification of Air by Metals.

THE researches of Elster and Geitel (*Phys. Zeitschr.*, v. p. 11) having led to the detection of radio-active power in the fine mud or "Fango" of the Italian watering-place Battaglia, induced me to undertake a study of the Russian muds in this character. Out of the five kinds of muds hitherto obtained by me and studied in a desiccated state, two muds, viz. that of the Odessa Kooyalnitzky Liman and that of Arensburg, on the Isle of Oesel, have proved undoubtedly to possess radio-activity, the first being radio-active in a higher degree than the second. In these researches we proceeded in a manner quite analogous to that of Messrs. McLennan and Burton in studying the electrical conductivity of the air (*Phil. Mag.*, v. p. 699, 1903). The present experiments were carried out with the most active participation of Mr. Athanassieff.

We employed two cylinders, a brass one 8·3 cm. in diameter and 20 cm. high, and another of zinc, 22·5 cm. in diameter and 35 cm. high. In each cylinder there was fixed along the axis a brass wire which was supported by an amber cylinder placed in a brass guard tube. The latter was connected with the earth, and embedded in an insulating ring put into an opening of the upper base of the cylinder. The wire of either cylinder could at will be connected with one pair of quadrants of a Dolezalek electrometer, the other pair being connected with the earth, and the leaf was charged to 100 V by means of a battery of storage cells. The wire, also of brass, connecting the cylinder wire with the electrometer, and the point of connection of that wire

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with either cylinder wire, were surrounded by brass tubes connected with the earth. The sensitiveness of the electrometer was such as to produce for a $\Delta V$ of 0·01 V, a shifting of about 7 mm. on the scale. The connection of the cylinder wires with the earth was brought about by touching them with brass wires connected with the earth. In the whole there was not on the path from the cylinder wire to the electrometer any contact of two different metals. The cylinder was charged, as a rule, to 100 V by a battery of storage cells.

During the progress of these experiments a very interesting phenomenon presented itself. It was found that when either of the cylinders is connected with the earth, the wire enclosed within it, after being disconnected from the earth, immediately begins to get electrified, *i.e.* the electrometer thereupon indicates a rise of a potential, which continually increases during a certain interval of time, some hours in the main, before reaching a limiting value. (The electrification was observed when the mud was removed from the cylinders.) The wire contained in the zinc cylinder becomes positively electrified, whilst that in the brass cylinder becomes negatively electrified. Having remarked such a phenomenon, we introduced into the brass cylinder which opened from beneath, a zinc cylinder, placed coaxially so as to enclose the wire. This cylinder was in metallic connection with the surrounding brass one. In this case, too, the wire acquired a potential, but it was opposite in sign to that it acquired without such a zinc cylinder being merely enclosed in the brass cylinder, *i.e.* it became positively electrified. The maximum value of the potential produced in the wire amounted in our observations to 0·2 V. This maximum value depends, it seems, upon the degree of ionisation of the air in the cylinder.

We also replaced the zinc cylinder at the interior of the great brass cylinder by others of lead, aluminium, iron and silver, with the effect that the two former acted in the same direction as the zinc cylinder; the lead cylinder, which, by the bye, proved very radio-active, gave the strongest effect (about 0·35 V), whilst aluminium took the last place, zinc remaining in the middle. The iron and the silver cylinders, on the contrary, exerted the same action as the main brass cylinder, giving a negative electrification, but to a less degree.

The phenomenon we have observed seems to be in correspondence with effects produced in metals by air ionised with Röntgen rays (I. Borgmann and A. Gerchun, *C. R.*, cxxii. p. 378, 1896; Minchin, the *Electrician*, March 27, 1896; Rutherford, *Phil. Mag.*, xliii. p. 241, 1897). It may perhaps give the explanation of atmospheric electricity; and it is also of interest in the fact that here we take electrical energy directly from the air.

I. Borgmann.

Physical Institute, The University, St. Petersburg, May 9.

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### Graphic Methods in an Educational Course on Mechanics.

It is difficult to reconcile Mr. Milne's opening statement (Nature, May 5, p. 5) with the rest of his letter. He begins by venturing to think that no one will gainsay Mr. W. Larden's main contention (Nature, April 28, p. 607) that "analytical methods give a grasp of the principles of statics while graphical methods disguise them," and he goes on to give half a dozen instances confuting it. Mr. Larden wrote to elicit opinions from those who have taught mechanics, and as I have had only one pupil, a very troublesome one, namely, myself, I cannot think that my opinions are invited. But when Mr. Milne thinks that no one will gainsay the contention, the challenge is a wide one, and I deny it emphatically, and know that there are hundreds of men who will agree with me. These men are not teachers or mathematicians, but those who have to use mathematics for their profession or trade.

I have the highest admiration for all those to whom science is an end in itself. I fully appreciate the attitude of mind (the butt of so many jokes) which feels that mathematics and other sciences become degraded by useful applications. But for one true mathematician there are a thousand men to whom mathematics are but a means to an end. Many of these, like myself, are not mathematically minded (as Mr. Larden probably counts mathematics), and with the exception of Maxwell's "reciprocal figures" and a few others, we have had to work out graphical methods mainly for ourselves. Teachers are now coming round, or as Mr. Larden would put it, giving way, or as I would put it, waking up, and are recognising that analytical language, powerful as it is for research, is not paramount for explanation. "I believe," wrote Prof. J. Perry in his "Spinning Tops," "that there are very few mathematical explanations of phenomena which may not be given in quite ordinary language to people who have an ordinary amount of experience. In most cases the symbolical algebraic expression must be given first by somebody, and then comes the time for its translation into ordinary language."

I agree with the whole of Mr. Milne's letter except the first few words, and, like him, "I believe the best results will be obtained when the two methods are used side by side." Of my own acquaintances, about one in five prefer analytical methods, but the others have a diagram in their heads, if not before them on paper as a guide to bring it vividly before the mind (to borrow Mr. Larden's words). Mr. Larden concludes, "graphical work consumes an amount of time that seems out of proportion to the mental training and knowledge of principles gained." The title of his letter shows that he has "an educational course" in view, and *qua* education, "mental training and knowledge of principles" is the true and only object. His pupils should emerge as mathematicians. But those who have to use statics professionally would not hesitate to consume twice or thrice the time on a graphical method if it carries conviction of truth with it, as it does to two or three at least out of five of my acquaintances.

Mr. Larden dates his letter from Devonport, and this suggests that some of his pupils hope to become naval officers and not wranglers; that mathematics will be used by them in after life as a means to an end. Would he deny the use of a piece of string on a globe to explain "great circle sailing," or does he use a formula applicable generally to figures of revolution, of which the earth and Saturn's ring are particular cases? Sumner's method may be disguised in algebra, but it must be confessed that the famous "line" as discovered by him was a bit of pure graphics.

It may be impossible for Mr. Larden to appreciate the geometrical point of view, for my contentions are exactly the opposite of his first and fourth. For us non-mathematicians, "graphical methods give a grasp of the principles of statics, while analytical methods disguise them," and "analytical methods confuse learners of statics." The second contention, "Analytical methods must be mastered in any case," needs the addition of the words "by the help of diagrams." If there be any truth in the third contention, that "analytical methods connect statics with dynamics," it is of small importance if they fail to elucidate dynamics. Nature herself gainsays these contentions with the parabola of the fountain, the ripple of the pond, and the slope of the sand hill.

A. P. Trotter.

8 Richmond Terrace, Whitehall, S.W., May 13.

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Any educational course in mechanics should undoubtedly be based first of all on experiment. If such is the case, it is practically impossible for any student using "graphical methods" to make the wild "shots" referred to by Mr. Larden (vol. lxix. p. 607), who seems to have been very unfortunate in the kind of boy he has received from "a preparatory school"; or is it the boy who has been unfortunate in his previous training? Has Mr. Larden considered the possibility of the "method of teaching" adopted being wrong in the aforesaid school? Surely there is no inherent quality in "graphical methods" to cause these wildest of "shots." The writer's experience goes entirely against this idea, and supports the contentions set forth by Mr. Milne.

Mr. Larden writes:—"If then, there be not time for both, it is the latter (Graphics) that should be sacrificed." If time is so short that some sacrifice must be made, the

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